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### Assessing Evidence

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# ASSESSING EVIDENCE

*Richard D. Friedman\**

STATISTICS AND THE EVALUATION OF EVIDENCE FOR FORENSIC SCIENTISTS. By *C.G.G. Aitken*. Chichester: John Wiley & Sons, Ltd. 1995. Pp. xiii, 260. \$59.95.

INTERPRETING EVIDENCE: EVALUATING FORENSIC SCIENCE IN THE COURTROOM. By *Bernard Robertson* and *G.A. Vignaux*. Chichester: John Wiley & Sons, Ltd. 1995. Pp. xxi, 240. \$55.

EVIDENTIAL FOUNDATIONS OF PROBABILISTIC REASONING. By *David A. Schum*. New York: John Wiley & Sons, Inc. 1994. Pp. xviii, 545. \$74.95.

Dwyer is accused of burglary. The police have found on his shoes several fragments of glass that have a mean refractive index close to — but not identical to — the mean refractive index of fragments of a broken window at the scene of the burglary.

The police also have found a blood stain near the broken window. Police investigators have subjected to DNA analysis both this stain and a sample of blood that they have taken from Dwyer. For each sample, this analysis attempts to measure the length of fragments at several identifiable sites, or loci, on the DNA molecule. The measurements for the two samples are very close — though not identical — for each locus.

How should the significance of this evidence be assessed?

## INTRODUCTION

Over the past few decades the law reviews, and more recently the Internet, have borne extensive discussions of whether, how, and how much the conventional theory of probability and alternatives to it are useful in modeling and analyzing problems in the law of evidence.<sup>1</sup> The discourse often has been abstract, but, as the hypo-

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\* Professor of Law, University of Michigan. B.A. 1973, J.D. 1976, Harvard; D.Phil. 1979, Oxford. — Ed. I have found communications with Colin Aitken, David Balding, and Peter Tillers, and especially very extensive communications with David Kaye and Peter Donnelly, extremely helpful in preparing this review. They have saved me from many errors, and I am grateful to them for their generosity of time and spirit.

1. See, e.g., Ronald J. Allen et al., *Probability and Proof in State v. Skipper: An Internet Exchange*, 35 JURIMETRICS J. 277 (1995); Symposium, *Decision and Inference in Litigation*, 13

thetical above suggests, it also has important consequences for determining how evidence is presented and assessed in litigation.

David A. Schum's *Evidential Foundations of Probabilistic Reasoning*,<sup>2</sup> C.G.G. Aitken's *Statistics and the Evaluation of Evidence for Forensic Scientists*,<sup>3</sup> and Bernard Robertson and G.A. Vignaux's *Interpreting Evidence: Evaluating Forensic Science in the Courtroom*<sup>4</sup> all have something to tell us about how to use and evaluate evidence. Although the books are addressed to different primary audiences<sup>5</sup> and their authors come from a variety of disciplines and from distant points of the English-speaking world,<sup>6</sup> all three help draw the connection between underlying theory and presentation in the courtroom. Though Schum uses numerous examples from litigation and discusses the legal literature of probability and evidence, he focuses primarily not on forensic matters but on the broader question of inference "in our work and in other parts of our daily lives" (Schum, p. xiii). Accordingly, he examines in depth the structure of inference, emphasizing conventional probability theory and alternatives to it. Aitken and Robertson and Vignaux, by contrast, essentially assume that the conventional theory is valid and apposite. They concentrate on the question of how to apply that theory in assessing and presenting evidence, especially scientific evidence, in court.

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CARDOZO L. REV. 253 (1991); Symposium, *Probability and Inference in the Law of Evidence*, 66 B.U. L. REV. 377 (1986).

2. David A. Schum is a Professor of Information Technology and Systems Engineering and a Professor of Law at George Mason University.

3. C.G.G. Aitken is a Senior Lecturer in the Department of Mathematics and Statistics at the University of Edinburgh.

4. When the book was published, Bernard Robertson was a Senior Lecturer in the Department of Business Law at Massey University, New Zealand. He since has become Editor of the New Zealand Law Journal. G.A. Vignaux is a Professor of Operations Research at the Institute of Statistics and Operations Research, Victoria University of Wellington, New Zealand.

5. Schum characterizes his audience as "students, researchers, and practitioners in every discipline in which there is concern about evidence and its inferential use." Schum, p. xv. Aitken's book is "intended for forensic scientists as well as statisticians," though he recognizes that some portions might be too difficult for forensic scientists and other portions might be too elementary for statisticians. Aitken, p. xii. Robertson and Vignaux say that their book "is designed to be read by both lawyers and forensic scientists so that each will better understand the other and they will be better equipped to work together to explain the evidence to the court." Robertson & Vignaux, p. xii.

6. Aitken is a Scottish statistician. Schum, an American with a Ph.D. in psychology and statistics, who defies easy categorization, is in part a systems engineer. Robertson and Vignaux are both Englishmen now living in New Zealand; Robertson, a former policeman, and a barrister at the Inner Temple of London, is a legal academic, and Vignaux, a physicist by training, is a professor of operations research. See *supra* notes 2-4.

I must admit to a certain bias: I know all the authors personally and like them all. All have been helpful to me professionally, and two have been gracious hosts to my family and me. If I thought the books had little value, I would not be able to write a trashing review. Luckily, I do not have that problem. Indeed, I happily provided a blurb for the dust jacket of the Robertson and Vignaux book, and I happily would have done the same for the others.

These three books, very different from one another, are all important works. Anyone wishing to think carefully about the nature of inference should study Schum's book. The reader is sure to find her fundamental conceptions — whatever they may be — seriously challenged. *Evidential Foundations of Probabilistic Reasoning* represents the culmination of many years of Schum's work, and it also helpfully summarizes much other scholarship from an extraordinarily wide variety of fields.<sup>7</sup> Litigators and academics interested in evidence, especially scientific evidence, should absorb the arguments that Robertson and Vignaux advocate vigorously and accessibly and that Aitken supports more technically.<sup>8</sup> In many contexts, a lawyer presenting scientific evidence in court should be sure that his forensic scientist can apply and present the statistical techniques set out by Aitken — or, if not, that he has a statistical expert who can.<sup>9</sup>

I cannot hope in this essay to summarize all the significant aspects of these three books. Rather, I will comment on selected themes. In Part I, I discuss briefly the usefulness of standard probability theory for analyzing legal inference. In Part II, I show why, as all the authors agree, the likelihood ratio is an important concept within probability theory for assessing the probative value of evidence. In Part III, I discuss theoretical problems, some highlighted by the authors, concerning the likelihood ratio. I conclude in Part IV by discussing consequences for the presentation of evidence in litigation.

## I. THE CONVENTIONAL THEORY OF PROBABILITY AND ITS ALTERNATIVES

I will begin with a primer on some principles of the conventional theory of probability and its application to problems of inference. This discussion may strike a median, not necessarily a happy one, between readers who find it unduly elementary and those who find it intimidatingly mathematical. Perhaps, though, some readers in the first group will find the diagrammatic technique interesting, even though the substance is hardly novel. I hope also that some readers in the second group will realize, with a little work, that the mathematics are really quite elementary.

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7. I do think the book is longer than need be; I suspect a tighter organization would have been possible.

8. The drama-to-content ratio is higher in the Robertson and Vignaux book than in Aitken's. For some readers, the ideal would be somewhere in the middle.

9. Most of Aitken's analyses are, I assume, persuasive at least to statisticians of a Bayesian inclination, but some are more controversial. See David J. Balding, *Book Review*, 15 INTL. STAT. INST., SHORT BOOK REVIEWS 41 (1995).

The theory begins with some basic premises. The probability of any event or proposition is assessed between 0 and 1, with 0 representing impossibility and 1 representing certainty. The probabilities of mutually exclusive events are additive — that is, if  $E$  and  $H$  both cannot be true,  $P(E \text{ or } H) = P(E) + P(H)$ . Because  $E$  and  $\text{Not-}E$  both cannot be true, but one or the other *must* be true,  $P(\text{Not-}E) = 1 - P(E)$ . And  $P(H|E)$ , the probability of  $H$  given that  $E$  is true, may be expressed this way:

$$P(H|E) = \frac{P(E \text{ and } H)}{P(E)} \quad (1)^{10}$$

Figure 1 may help us explore Equation 1.

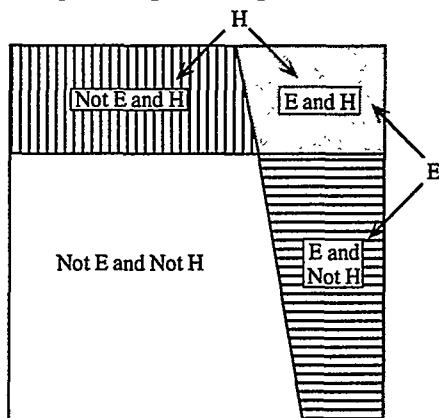


FIGURE 1

The figure is a box the area of which we may deem to equal 1. A portion of the box is labeled  $H$  and marked with vertical lines. Another portion, which overlaps in part, is labeled  $E$  and bears horizontal marks. Thus, the overlapping portion is labeled  $E \text{ and } H$  and has crossed marks.  $P(H)$ , the probability that a point picked at random from the entire box will be within  $H$ , is simply the area of  $H$ ; similarly,  $P(E)$  equals the area within  $E$ , bearing horizontal marks, and  $P(E \text{ and } H)$  equals the area of the overlap.

Now suppose we want to assess the probability of a hypothesis — that a certain point, randomly selected, falls within  $H$  — and we are given a piece of evidence, that the point falls within  $E$ . Given this information, we need not concentrate on the entire box; rather, we should narrow our focus to the area with horizontal marks, representing the truth of  $E$ . *Within* that area,  $H$  is true only within the smaller area bearing crossed marks. The probability of  $H$ , given the truth of  $E$ , is thus equal to the ratio of  $P(E \text{ and } H)$  to  $P(E)$  — that

10. See Robertson & Vignaux, pp. 14-15; Schum, pp. 38-43.

is, the ratio of the probabilities of  $E$  and  $H$  and of  $E$  alone, respectively, both assessed before the truth of  $E$  was known.

Because Equation 1 speaks in general terms, the variables  $E$  and  $H$  may be reversed. That is,

$$P(H|E) = \frac{P(H \text{ and } E)}{P(E)}. \quad (2)$$

And because  $P(E \text{ and } H)$  equals  $P(H \text{ and } E)$ , Equations 1 and 2 may be combined to yield

$$P(E \text{ and } H) = P(E) \cdot P(H|E) = P(H \text{ and } E) = P(H) \cdot P(E|H),$$

and so 
$$P(H|E) = P(H) \cdot \frac{P(E|H)}{P(E)}. \quad (3)$$

Equation 3 is a form of what is known as Bayes's Theorem (or Rule), and it deserves close examination. The factfinder's task is to determine  $P(H|E)$ , the probability of the hypothesis given the evidence. In some cases it will be easier to assess  $P(E|H)$ , the probability of the evidence given the hypothesis, which may provide a practical basis for assessing  $P(H|E)$ . Bayes's Theorem thus provides a means for *transposing the conditional*, using  $P(E|H)$  to help determine  $P(H|E)$ .

Bayes's Theorem also may be stated in another compact form, one particularly helpful in discussing the theme of these books. Substituting *Not- $H$*  for  $H$  in Equation 3, we have

$$P(\text{Not-}H|E) = P(\text{Not-}H) \cdot \frac{P(E|\text{Not-}H)}{P(E)}. \quad (4)$$

Dividing Equation 3 by Equation 4, and simplifying slightly, yields

$$\frac{P(H|E)}{P(\text{Not-}H|E)} = \frac{P(H)}{P(\text{Not-}H)} \cdot \frac{P(E|H)}{P(E|\text{Not-}H)}. \quad (5)$$

The odds of a proposition  $X$  are defined to be  $P(X)/[1 - P(X)]$ , or  $P(X)/P(\text{Not-}X)$ . Thus, a probability of .5, or  $\frac{1}{2}$ , corresponds to *even odds* of 1. The left-hand side of Equation 5 is therefore  $O(H|E)$ , the odds of  $H$  given  $E$ , and the first fraction on the right-hand side is  $O(H)$ , the odds of  $H$  assessed before knowledge of whether  $E$  is true. The second fraction on the right-hand side is the *likelihood ratio* of  $E$  with respect to  $H$ , which I will denote as  $L_H(E)$ . It indicates how much more (or less) likely the evidence  $E$  will appear given the hypothesis  $H$  than given the negation of that hypothesis. Equation 5 now may be rewritten as

$$O(H|E) = O(H) \cdot L_H(E). \quad (6)$$

In other words, to determine the *posterior odds* of  $H$  given  $E$ , we can begin with the *prior odds* of  $H$  assessed before we learned whether  $E$  was true, and then multiply by the likelihood ratio. If  $E$  is more likely to be true given  $H$  than given *Not- $H$* , then proof of  $E$

increases the odds of  $H$ ; by contrast, if  $E$  is less likely to be true given  $H$  than given  $Not-H$ , then proof of  $E$  decreases the odds of  $H$ ; and if  $E$  is equally likely to be true given  $H$  and given  $Not-H$ , then proof of  $E$  is irrelevant to  $H$ , leaving the odds of  $H$  unchanged.

Again, Figure 1 may help demonstrate this relationship. Before we know whether  $E$  is true, the odds of  $H$  are the ratio of the area above the border of  $H$  to the area below it,  $P(H)/P(Not-H)$ . Once we know that  $E$  is true, we must confine our attention to the area to the right of the diagonal line; *within that area*, the odds of  $H$  are again the ratio of the area above the border of  $H$  to the area below it,  $P(E \text{ and } H)/P(E \text{ and } Not-H)$ . That is, the proof of  $E$  means that we substitute the smaller area ( $E \text{ and } H$ ) for the larger area  $H$  in the numerator of the odds ratio, and the smaller area ( $E \text{ and } Not-H$ ) for the larger area  $Not-H$  in the denominator. Put another way, adjusting the odds of  $H$  in light of  $E$  requires us to multiply the numerator by  $P(E \text{ and } H)$  and divide it by  $P(H)$ , and to multiply the denominator by  $P(E \text{ and } Not-H)/P(Not-H)$ . By applying Equation 2 to these expressions, we can see that this means more simply that we multiply the prior odds by  $P(E|H)/P(E|Not-H)$ , which is the likelihood ratio.

In Figure 1, because the line marking off  $E$  runs diagonally down from left to right, a higher proportion of the  $H$  area than of the  $Not-H$  area is within  $E$ ; that is,  $P(E|H)$  is greater than  $P(E|Not-H)$ , which means that the likelihood ratio is greater than 1. Thus, the posterior probability  $P(H|E)$  is greater than the prior probability  $P(H)$ , meaning that the evidence tends to support the hypothesis. In Figure 2,  $E$  is marked off by a vertical line. The like-

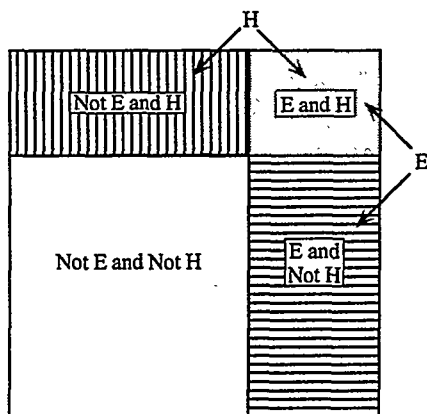


FIGURE 2

likelihood ratio is 1, and the evidence is irrelevant to the hypothesis. In Figure 3,  $E$  is bordered by a diagonal line running down from right

to left, so the likelihood ratio is less than 1 and the evidence tends to disprove the hypothesis.

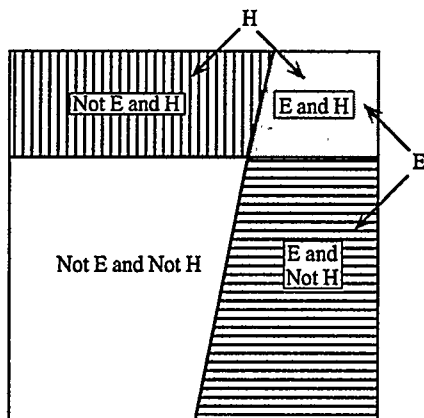


FIGURE 3

All this may seem to have little to do with real evidence and hypotheses, for most often probability assessment is a highly subjective matter. Most rational observers will agree that the probability is .5 that a fair coin will land heads up on any given flip, or that the probability is .4 that the head of a pin dropped at random over a given area will land within the top 40% of that area. But reasonable observers may disagree as to the probability that a team from the American Football Conference will win the next Super Bowl, or the probability that Lee Harvey Oswald acted alone in shooting President Kennedy. Nevertheless, as Schum shows, probability theory still applies to subjective assessments of probability: the theory allows an observer to accord any probability assessment to propositions such as these, but continues to impose consistency conditions on such assessments. Thus, an observer rationally could not assess as .6 both the probability that a given proposition is true and the probability that it is false (Schum, pp. 53-54).

I have presented here only the beginning stages — those that will be helpful for later parts of my discussion — of an approach to inference based on the application of the conventional theory of probability to subjectively determined probability assessments. Because of the prominence of Bayes's Theorem in this approach, it is often labeled Bayesian, and though the label is potentially misleading I will use it here.<sup>11</sup> Schum takes the analysis much further, ex-

11. The label may be misleading for at least two reasons. First, Bayes's Theorem is an aspect of probability theory that is not limited to a subjective concept of probabilities; as a mathematical derivation from premises of the theory, it is not controversial. Second, use of Bayes's Theorem is only one aspect of the approach to inference based on subjective probabilities. It is an essential tool for updating probability assessments in some situations, but in some it is not and in some it is not sufficient. See generally D.H. Kaye, *Introduction*:



amining closely some complicated patterns of inference<sup>12</sup> and revealing the complexities in what superficially might appear to be a relatively uncomplicated inference.<sup>13</sup> Just as a microscopic view may reveal that an apparently simple surface is in fact highly complex, the evidence-hypothesis link may be decomposed to a far finer granularity than in the discussion above. Entire bodies of evidence, not simply one item, must be considered simultaneously. Also, the problem may require analysis not just of *H* and *Not-H*, but of a large, even infinite, number of potential hypotheses.

The analysis is, as Schum says, "very rich in its implications," but sometimes so rich as to cause "intellectual indigestion" (Schum, p. 244). As Schum demonstrates, the principal problems are that a rigorous Bayesian approach often requires assessing many probabilities, creating enormous computational complexity, and that as to at least some of these probabilities an observer will have only a flimsy basis for making the assessment (Schum, pp. 343-44).

The flimsiness problem does not strike me as a serious one: if we have enough knowledge to understand a proposition, we have enough knowledge to make a guess — however tentative — as to how probable it is. To use Schum's terms, I do not believe "total evidential vacuities exist."<sup>14</sup> Suppose, for example, you are sitting in a restaurant when you hear a voice that you do not recognize yell, "There's been an accident outside!" You know nothing about the declarant and her relationship to what either did or did not happen outside, apart from what you know in general about the world and what you can infer from her voice. But you know enough to make a preliminary assessment of how likely she would make the statement if it were true, and how likely she would do so if it were not — that is, you have enough information to make an assessment, albeit very tentative, of the likelihood ratio. If you turn to look and find that she is obviously drunk or obviously joking or, on the other hand, shaken and bloodied, you rapidly and radically may reassess the likelihoods in light of this further information.

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*What is Bayesianism?*, in *PROBABILITY AND INFERENCE IN THE LAW OF EVIDENCE: THE LIMITS AND USES OF BAYESIANISM 1* (Peter Tillers & Eric D. Green eds., 1988), reprinted in D.H. Kaye, *What is Bayesianism? A Guide for the Perplexed*, 28 *JURIMETRICS J.* 161 (1988).

12. See, e.g., Schum, pp. 382-90, 412-17.

13. See, e.g., Schum, pp. 324-33, 346-50.

14. See Schum, p. 344. One especially perceptive commentator has noted:

It is . . . difficult to conceive of any question that can be answered by scientific investigation for which a person of more than a few years of age who has not been reared in a state of total sensory deprivation would have *no* "empirical data." The data may be extremely limited, highly peripheral, or grossly faulty, but there will almost always be some information on which to base a subjective probability.

David Kaye, *The Laws of Probability and the Law of the Land*, 47 *U. CHI. L. REV.* 34, 44 n.37 (1979).

Computational complexity poses a significant obstacle to any theory that assumes that observers can, or should, go through a complete Bayesian analysis whenever they face an inferential problem. I do not take that position, nor do I know of any commentator who does. Observers tend to simplify a situation, I have suggested, as "a way to make the problem intellectually tractable and the demand for information economically feasible."<sup>15</sup> If applied to take into account all the information we have about a situation, Bayesian analysis requires unrealistically complex calculations, but this does not suggest a problem with the theory. On the contrary, the complexity is in the world surrounding us, and the theory would have limited value if it could not in principle represent that complexity. Probability theory is a flexible template. It can take into account as much complexity as its user is able to handle. But if constraints on the user's capacity mean that, at the cost of ignoring information, she must distill much of the complexity out of the situation — as I have done in the discussion above — the theory is equally applicable.

Indeed, I do not believe that observers usually need to assess probabilities consciously, or numerically, to make sound inferences.<sup>16</sup> I contend only that if we are thinking well — and plainly we do not always do so<sup>17</sup> — we reach, or perhaps move toward, an equilibrium based at least implicitly on probability assessments that are roughly consistent with Bayesian principles, applied with the aid of whatever simplifications we find necessary. In this view, rational thinking does not require that we actually achieve this Bayesian equilibrium, only that if at a given time we have inconsistent probability assessments we make adjustments in the direction of eliminating the cognitive dissonance.<sup>18</sup>

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15. Allen et al., *supra* note 1, at 295, quoted in *United States v. Shonubi*, 895 F. Supp. 460, 487 (E.D.N.Y. 1995).

16. Figure 1 presents a nonnumerical representation of probabilities by the relative sizes of areas. Similarly, if we ask an observer how tall another person is the observer may give an assessment — even a rather precise assessment, whether right or wrong — by holding his hand a given distance off the ground. We might decide to measure that assessment numerically for our own purposes, but the observer does not necessarily need a numerification for his purposes — for example, to decide how high he must hide a jar of cookies to put them beyond reach.

Schum refers to the psychologist S.S. Stevens's "cross-modality matches" procedure, in which people use one sensory modality to express judgments about another — for example, adjusting the loudness of a sound to represent the brightness of a light. Schum suggests that one might extend this approach to expressing intensity of belief in a hypothesis by non-numerical means. See Schum, p. 224.

17. See generally DANIEL KAHNEMAN ET AL., *JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES* (1982).

18. Suppose that before knowing whether  $E$  was true, an observer assessed  $O(H)$  to be even — that is, 1:1. Then she receives proof that  $E$  is correct and assesses  $O(H|E)$  as 2:1 and  $L_H(E)$  as 3. There is a dissonance here, because these assessments violate Equation 6. The observer might respond to this dissonance by adjusting one or more of the assessments; sim-

Although most of Schum's discussion is orthodox Bayesian, he also presents three other systems of measuring partial belief. One of these, the system of so-called Baconian probabilities, which is associated with L. Jonathan Cohen,<sup>19</sup> strikes me as having little bearing on legal inference.<sup>20</sup>

About the only good thing that we can say for the Baconian approach, at least in the legal context, is that it "makes quite explicit the importance of the *completeness* of evidential coverage in any form of inductive reasoning" (Schum, p. 260). Although the standard of persuasion in a civil case often is expressed in nakedly probabilistic terms such as "more likely than not," there may be more to the standard than that. Perhaps a verdict for the plaintiff is not justified unless the court<sup>21</sup> believes not only that the probabilities are on the plaintiff's side but also that it has been presented with a sufficiently complete picture of the facts in dispute to warrant overcoming inertia and ordering relief. This possibility, it seems to me, lies at the heart of the well-worn discussion of the so-called "Blue Bus" hypothetical and related chestnuts.<sup>22</sup> In other words, while the standard of persuasion includes a probability component that must be conceived of in Bayesian terms, perhaps there is also a separate *completeness* component.<sup>23</sup> If so, it may be that

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ply because  $O(H)$  is labeled *prior odds* does not mean that she cannot adjust it later when she focuses on the consequences of that assessment. And it may be that the observer, rather than eliminating the dissonance immediately, will make incremental adjustments in the right direction. The process might be disrupted before equilibrium is reached.

19. See Schum, pp. 243-61; L. JONATHAN COHEN, *THE PROBABLE AND THE PROVABLE* (1977).

20. This is ironic given Cohen's emphasis on so-called anomalies and paradoxes in legal reasoning. See Schum, p. 244. I believe that Cohen misunderstands legal reasoning. See Richard D. Friedman, *A Diagrammatic Approach to Evidence*, 66 B.U. L. REV. 571, 575 n.11 (1986); see also Dale A. Nance, *A Comment on the Supposed Paradoxes of a Mathematical Interpretation of the Logic of Trials*, 66 B.U. L. REV. 947 (1986). I doubt that many lawyers will find persuasive Schum's statement, relying only on works of Cohen himself, that Cohen's ideas "are readily applicable in fields such as law." Schum, p. 261.

21. For present purposes, it is not necessary to distinguish between the court and the jury.

22. See, e.g., James Brook, *The Use of Statistical Evidence of Identification in Civil Litigation: Well-Worn Hypotheticals, Real Cases, and Controversy*, 29 ST. LOUIS U. L.J. 293 (1985). Under one common version of this hypothetical, plaintiff is injured by a negligently driven bus, but the only evidence she is able to offer to identify the owner of the bus is proof that 80% of the buses driven in the town belong to the Blue Bus Company. The question is whether this evidence is sufficient to support a verdict for the plaintiff.

23. Incompleteness of evidence with respect to a proposition in itself tends to prevent an observer from assigning a high probability to the proposition because the observer should consider the possibility that the missing evidence would, if produced, tend to negate the proposition. See D.H. Kaye, *Do We Need a Calculus of Weight To Understand Proof Beyond a Reasonable Doubt?*, 66 B.U. L. REV. 657 (1986) (relying on this point in attempting to develop a model of forensic proof that relies exclusively on conventional probability theory and yet accomplishes the tasks for which an independent concept of weight might be thought necessary). It may be, however, that a verdict finding a particular proposition to be true will appear to us more warranted, even given the same probability assigned to the proposition by the factfinder, if the completeness of the evidence surpasses some threshold. For another

this completeness component cannot be measured in Bayesian terms, at least not simply.

Baconian probabilities, though, do not reflect incompleteness in a way that is useful for legal analysis. They are ordinal, not cardinal;<sup>24</sup> they rely on a system of eliminative induction — in which the grading of probability depends on how many tests, out of how many possible ones, a given proposition has passed — that has no resemblance to legal factfinding;<sup>25</sup> and they do not tolerate the simultaneous partial belief in contradictory propositions (Schum, p. 254).

More promising, I believe, is the system of belief functions developed by Glenn Shafer,<sup>26</sup> which Schum presents lucidly (Schum, pp. 225-43). This system is, in a sense, a generalization of the Bayesian one,<sup>27</sup> and it suffers none of the disabilities of the Baconian system described above. But, whereas in the Bayesian system  $P(H) = 0$  means that  $H$  is impossible and no other evidence can alter this probability assessment, in Shafer's system  $Bel(H)$ , the belief in  $H$ , may be revised upward from 0.<sup>28</sup> Similarly,  $Bel(H) + Bel(Not-H)$  does not necessarily equal 1 (Schum, p. 238). If, therefore, the standard of persuasion does incorporate a component of completeness, this system may provide a useful basis for thinking about whether a given body of evidence satisfies that requirement.<sup>29</sup> On the other hand, the mechanics prescribed by Shafer's system for altering beliefs in light of multiple pieces of evidence (Schum, pp. 238-42) — a problem the Bayesian system handles with ease — strike me as

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tentative development of this point, see my comments in Allen et al., *supra* note 1, at 299, 310.

24. See Schum, p. 251. One indication that the legal system operates on a system of cardinal probabilities is provided by FED. R. EVID. 403, which requires the court to weigh the probative value of evidence — that is, the *extent* to which it alters the probability of a material fact — against various considerations, including the danger of unfair prejudice.

25. Thus, Schum's most extensive development of the Baconian system is in a problem of electronics system analysis, not one of legal fact finding. See Schum, pp. 245-49. Schum's book, it bears noting, is part of the Wiley Series in Systems Engineering.

26. See GLENN SHAFER, A MATHEMATICAL THEORY OF EVIDENCE (1976).

27. Suppose a set of three mutually exclusive and exhaustive hypotheses,  $H_1$ ,  $H_2$ , and  $H_3$ . In the Bayesian system,  $P(H_1) + P(H_2) + P(H_3)$  must equal 1. Shafer's system also requires a prescribed sum to equal 1, but the requirement is a looser one, applying to the support allocated among subsets of hypotheses. That is, an observer might say that  $s[H_1]$ , the support for  $H_1$ , is .1,  $s[H_2] = .2$ ,  $s[H_3] = .3$ , and  $s[H_2, H_3] = .4$ . The belief accorded to any subset is then the sum of the supports for that subset and for any smaller subset that necessarily entails it. Thus,  $Bel[H_2, H_3] = s[H_2] + s[H_3] + s[H_2, H_3] = .2 + .3 + .4 = .9$ , and  $Bel[H_1, H_3] = .1 + .3 = .4$ , but  $Bel[H_2]$  is simply .2, because the only nonempty subset of  $[H_2]$  is  $[H_2]$  itself. The doubt about a subset is defined to equal the belief of the negation of that subset; because  $[H_2]$  is the negation of  $[H_1, H_3]$ ,  $Dou[H_1, H_3] = .2$ . And the plausibility of a subset is defined to equal 1 minus any doubt about that subset; thus  $Pl[H_1, H_3] = .8$ .

28. See Schum, p. 252. Similarly, Baconian probabilities may be revised upward from 0.

29. Cf. Schum, p. 238 ("Neither posterior nor prior probabilities alone say anything about the weight of evidence. In Shafer's system there is a relationship between evidential support and evidential weight.").

rather unappealing.<sup>30</sup> Schum's discussion of Shafer's system is too brief to make clear whether this is indeed a flaw so critical that it nullifies the potential usefulness of the system; thus Schum leaves me wanting to know more about the Shafer system.

The same goes for the final system Schum discusses, the fuzzy logic developed by Lotfi Zadeh. The basis of this system is the notion of fuzzily defined sets. To use one of Schum's examples, we might say that a person age forty-six is neither entirely in nor entirely out of the set of old persons, but is fifty-five percent in the set (Schum, p. 262).<sup>31</sup> This system may well have interesting uses in legal reasoning, and I suggest below one possibility, but Schum's explanation is tantalizingly brief and sketchy.

Each of these systems, Schum believes, has some merit; each "allows us to discern different attributes of evidence and its inferential use" (Schum, p. 510). Perhaps so, but I wish Schum would reserve the term *probability* for the conventional theory. Other systems may be useful in grading some other aspects of partial belief, but it seems to me that those aspects are not probability. Indeed, the very fact that those systems operate in a different manner from the conventional one, grading different attributes, gives them whatever value they might have. Near the end of his book, Schum concedes that he has found his "Bayesian lens" more frequently useful than the others (Schum, p. 510). The reason, he contends too modestly, is in part his own greater familiarity with the Bayesian system.<sup>32</sup> He does allow, however, that one inherent advantage of the Bayesian system may account in part for his preference.<sup>33</sup>

The Bayesian system is not contrived. Even to the extent it is only aspirational, it reflects rational treatment of uncertainty based on intuitively appealing and experientially successful premises. There is much to be said for the argument by Robertson and Vignaux that instead of speaking of the Bayesian approach we might use the term *logical*, because "the method is essentially a generalisation of ordinary logic" (Robertson & Vignaux, p. 114).

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30. The method bears striking resemblance to an early method for analyzing credibility that I criticized in Richard D. Friedman, *Route Analysis of Credibility and Hearsay*, 96 YALE L.J. 667, 674-75 (1987).

31. See Schum, p. 262. Rapidly approaching that age myself, I might have guessed that a lower percentage would be appropriate.

32. Schum is not only modest, but as all who know him can attest, he is an extraordinarily nice man. I think that, like a deist, he looks for good in everything; such a pleasantly receptive attitude to people and the world might account in part for what he calls his "obstinate pluralism." See Schum, p. 510.

33. He points to the feature of conditional nonindependence. See Schum, p. 510. It may be, for example, that given *H*, proof of *E* makes *F* more likely, but that given *Not-H*, proof of *E* has no impact on the probability of *F*.

## II. THE LIKELIHOOD RATIO

All three books stress the importance of the likelihood ratio within the Bayesian system. Though Schum's interests transcend the legal system, he advocates using the likelihood ratio to measure the value of evidence and shows carefully how to calculate it in various stylized contexts (Schum, pp. 218-20, 292-446). Aitken declares that the "main theme" of his book "is that the evaluation of evidence is best achieved through consideration of the likelihood ratio" (Aitken, p. xii). "The likelihood ratio," he says, "may be thought of as the *value* of the evidence," (Aitken, p. 49), and much of his book is devoted to showing how to calculate this ratio with respect to various types of evidence that a forensic scientist might offer in court. Robertson and Vignaux address a far less technical audience and so concentrate far less on calculating likelihood ratios. To a large extent, their book is a work of advocacy for the proposition that expert witnesses generally ought to testify in terms of the likelihood ratio. They would prefer that experts testify in terms such as the following: "This evidence is  $R$  times more probable if the accused left the mark than if someone else did. This evidence therefore [very] [strongly] supports the proposition that the accused left the mark."<sup>34</sup>

Part I already has discussed the principle that the likelihood ratio, when measured by the prior odds of a hypothesis, yields the posterior odds of the hypothesis. Thus, Robertson and Vignaux suggest that, as an alternative to the conclusion of the model testimony above, the witness could testify: "Whatever odds you assess that the accused was present on the basis of other evidence, my evidence multiplies those odds by  $R$ " (Robertson & Vignaux, p. 65).

I believe that people often use the likelihood ratio implicitly in ordinary reasoning. Take this illustration: While driving along on the highway, you wonder whether a nearby car is heading to the airport. Along with a minority of other cars, this one eventually takes the exit from the highway that leads most directly to the airport. You might reason implicitly as follows: If they were heading to the airport, they almost certainly would take this exit. If they were not heading to the airport, they probably would not take the exit.  $P(E|H)$  is therefore much higher than  $P(E|Not-H)$ , and so this evidence makes it significantly more likely that the car indeed is heading to the airport.

Note that while this evidence makes the airport hypothesis more likely than it was without the evidence, it does not necessarily make that hypothesis more likely than not. If, say, the people in the car

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34. Robertson & Vignaux, p. 65 (brackets in original); see also Robertson & Vignaux, p. 21.

all were dressed in bathing suits, you might have assigned very low prior odds to the proposition that the car was headed to the airport, and the turn off the highway would not necessarily make that proposition probable — especially if there were a swimming place along the same road as the one that leads to the airport.

It is important also to note that the logic of this Bayesian argument is *not*: “Most cars that leave the highway at this point are heading toward the airport. Therefore, I conclude that this car is probably heading toward the airport.” There are at least three problems with this kind of stereotyping argument.

First, the premise stated in the first sentence of the stereotyping argument is an overly demanding one, not necessary for the evidence to have significant probative value: the fact that the car takes the exit may tend strongly to prove that the car is on the way to the airport, even though only a minority of the cars that take the exit are heading toward the airport, because a far higher proportion of the cars that are heading to the airport than of those that are not take the exit.

Second, the stereotyping argument equates the probability that *this* car is headed toward the airport given that it is taking the exit with the probability that a car selected at random from all cars taking the exit is headed toward the airport. In doing so, the argument ignores the prior odds; that is, it ignores all other information we might have about the particular car, such as bathing suits worn by its occupants, that might make it appear significantly more or less likely, as compared with a randomly selected car, to be on its way to the airport.

Third, the argument does not form part of any useful comparison of stories, because it lacks any causative element. Schum argues elegantly and persuasively that no causative connection is necessary for evidence to be relevant to a hypothesis (Schum, pp. 140-56). Even so, I think we can assess probabilities more sensibly when we have a causative relationship in mind. Usually, a car either does or does not take a given highway exit because of a decision made by the person navigating as to how best to get to the intended destination. Thus, it makes causal sense to say: “If a car is heading to the airport, there is a high probability that it will take this exit (because that is the best way to get to the airport), and if it is not headed to the airport, there is a low probability that it will take this exit (because that is not a good way to get to places other than the airport where people are most likely to want to go from here).” One would have to believe in purposeless navigation of the “follow-your-nose” type, or perhaps in anthropomorphized cars

with unrevealed intentions,<sup>35</sup> to treat as a causal statement, "If the car takes the exit, it probably will head toward the airport (because cars taking the exit tend to head that way)."<sup>36</sup>

This illustration shows how using the likelihood ratio allows us to combine new evidence with other information we have that bears on the hypothesis in question. This approach does not force us to treat the case as somehow typical of all those in which the evidence might arise. Moreover, it uses the explanatory force of the evidence given by the causal relationship between the hypothesis, or its negation, and the evidence.

Now I will vary the airport hypothetical somewhat. Suppose we are not on a highway but heading north on Central Boulevard. To get from there to the airport, one must take any of ten cross streets — it does not matter much which one — leading east to Airport Boulevard. The vast majority of cars on Central that are not headed to the airport pass the ten streets without turning onto any one of them. Now suppose a car on Central turns east onto the third of the cross streets. In this situation, we cannot say that the probability of this evidence given the airport hypothesis is high — that is, that the numerator of the likelihood ratio is nearly 1. Rather, this likelihood — the probability of turning onto that particular street given the airport as a destination — is presumably about .1. But this likelihood is presumably still far greater than the denominator of the likelihood ratio, the probability of the evidence given the negation of the airport hypothesis — that is, the probability that the car would take that street given that it was not headed to the airport. As before, the likelihood ratio is substantially greater than 1, and the evidence significantly favors the hypothesis that the car is headed toward the airport.

The general insight is that evidence may tend to prove a given hypothesis, even though it is unlikely to arise given that hypothesis, if it is even less likely to arise given the negation of that hypothesis. This insight is helpful in thinking about scientific evidence. As suggested by the burglary hypothetical at the beginning of this review,

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35. A personal note: When I was very small, my father often said to me, "Let's see where the car takes us." Almost inevitably, except for one disastrously premature trip to the late, lamented Ebbets Field, the car took us to the late, lamented Nunley's Carousel. *See generally* Pam Belluck, *Nostalgia is Power in Fight Over Carousel*, N.Y. TIMES, Sept. 18, 1995, at B6. In recent years, I have come to believe that my father misled me, in that he directed the car rather than the other way around.

36. Of course, it would be a fair causal statement to say: "I infer that the car is headed towards the airport because it is taking this exit, and this, in light of all the other information I know, suggests the airport as the destination." This, however, is a causal statement about the inference itself, not about events on the road. Similarly, my three-year-old daughter sometimes says, "I'm tired, because I yawned." My wife interprets this conclusion as reflecting illogic and ignorance: yawning is not a substantial cause of fatigue. More charitably, I interpret the statement as shorthand for: "Given the fact that I yawned, and in light of all other information that I have, I infer that I am tired."



samples taken from the same pane of glass are likely to have, and to be measured as having, a variety of different indices of refraction (Aitken, pp. 174-75; Robertson & Vignaux, p. 115). And, though a person's DNA is the same virtually throughout his body, the situation is largely similar. Current technology employs remarkably indirect methods to determine DNA profiles, so multiple samples taken from the same person — and even multiple tests of the same sample — will yield a variety of slightly different profiles (Aitken, pp. 216-17, 230-34; Robertson & Vignaux, p. 129). The probability of an exact match between two measurements, even though the samples come from a common source, may be very small; consequently, the fact that the measurements of two samples fail to match exactly does not signify that the samples lack a common origin.

These statistical disparities create a very complex statistical problem, but they do not create a fundamental conceptual difficulty under the likelihood-ratio approach.<sup>37</sup> If  $H$  is the probability that the two samples have a common origin and  $E$  is the evidence of the measurement of the two samples, then it may be that the likelihood ratio,  $L_H(E)$ , is very large even though the numerator,  $P(E|H)$ , is quite small. We might assess the evidence in this way: "This precise result would be quite unlikely under the hypothesis that the two samples come from a common origin,<sup>38</sup> but it would be absolutely extraordinary under the hypothesis that the two samples did *not* come from a common origin. Therefore, the evidence makes more likely the hypothesis that the two samples come from a common origin."

Both Aitken and Robertson and Vignaux strongly endorse the likelihood-ratio approach and argue vigorously against the approach traditionally and most commonly used for DNA and glass evidence. Under this "two-stage" approach,<sup>39</sup> an observer first must determine whether the two samples match. A declaration of a match does *not* mean that the samples have a common source. Rather, it means only that the measurements are sufficiently close to each other to satisfy some prescribed standard. This standard is generally set by adapting the classical method of statistical infer-

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37. The pioneering exposition with respect to glass was presented by Dennis Lindley. See D.V. Lindley, *A Problem in Forensic Science*, 64 *BIOMETRIKA* 207 (1977). Aitken presents an analysis drawn from Lindley's. Aitken, pp. 179-81, 203.

38. In some cases we could add: "though perhaps it is not substantially more unlikely than any other precise result under that hypothesis."

39. I believe this term was coined by Ian Evett, a celebrated interpreter of forensic science employed by the British Home Office. For a lucid exposition of the traditional approach and an explanation of how he came to believe it is fundamentally wrong for application to forensic science, see I.W. Evett, *Interpretation: A Personal Odyssey*, in *THE USE OF STATISTICS IN FORENSIC SCIENCE* 9 (C.G.G. Aitken & D.A. Stoney eds., 1991).

ence<sup>40</sup> and is essentially arbitrary; if the standard is tight, a match will not be declared in a substantial percentage of cases in which the samples in fact come from a common source, and if the standard is too loose the declaration of a match will lose much of its probative value. If the two samples are declared to match, the second step is to determine the significance of the match, usually by determining the probability that a match would occur if one of the samples were chosen at random from a given population. In markedly different styles, Aitken and Robertson and Vignaux ably expose numerous problems with this approach (Aitken, pp. 92-106, 237-38; Robertson & Vignaux, pp. 114-20). Among them are the following:

(1) The concept of a match is gratuitous, an unnecessary intrusion between evidence and hypotheses.<sup>41</sup> Put in simplified terms, the factfinder's task is to assess the relative probability of two hypotheses — that the samples came from a common source, and that they did not. The evidence is the two profiles revealed by the samples. A factfinder can ask how likely it is that the evidence would have arisen, given each of the competing hypotheses, without asking whether the evidence satisfies an arbitrarily defined match standard.

(2) The artificial dichotomy of match-or-no-match produces an unhelpful bright-line, or "fall off the cliff," effect (Aitken, p. 237; Robertson & Vignaux, p. 118). Two samples are deemed to match, notwithstanding small differences between their measurements, as fully as if they were identical. If the differences surpass a given threshold, however, the samples are deemed not to match, just as if they bore no resemblance to one another at all. By contrast, the likelihood ratio is continuous: all other things being equal, the greater the difference between the measurements, the smaller the likelihood ratio.

(3) The match approach may declare no match even though the evidence, properly viewed, tends to prove that the samples come from a common source (Aitken, pp. 98-99; Robertson & Vignaux, p. 119). To prevent false positives, the match approach must declare no match in some cases in which the difference between the two profiles is unusually large for samples from a common source. In some of these cases, however, the differential is small enough that the coincidence would seem extraordinary if the samples did

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40. See Aitken, p. 211. A match is declared if the difference between the measurements is small enough that, if the samples were assumed to come from a common source, the difference would be expected to be as great or greater in a prescribed percentage of cases.

41. See Aitken, p. 106 (quoting with irony an earlier commentary that the likelihood ratio approach "is of rather limited use to forensic scientists, by-passing as it does the similarity question altogether"); Robertson & Vignaux, p. 120 ("In fact we do not need the concept of a match at all.").

not come from a common source. Thus, the likelihood ratio may be greater than 1 despite the declaration of no match.

(4) Robertson and Vignaux quite forcefully, and Aitken more subtly, suggest that evidence presented under the matching approach does not combine readily with other evidence in the case.<sup>42</sup> They seem clearly correct that the classical method of statistical inference is designed to test a hypothesis on the basis of data alone, without relying on subjective probability assessments (Robertson & Vignaux, pp. 114, 117). For scientific purposes, this method no doubt has great value. It poses difficulties in the litigation context, however, because judicial factfinders do not have the luxury of withholding decision until they have sufficient quantifiable data. Rather, they must combine the quantifiable evidence with the subjectively determined probability assessments they make on the basis of the other evidence in the case and the real-world information they are allowed to consider in order to make an updated subjective probability assessment.

This concern might have some theoretical force, in the context of DNA evidence, when a prosecutor presents evidence that two samples do not match because the disparity between the measurements is so great. Such a conclusion tells the factfinder that the evidence would be unlikely to arise given the hypothesis that the samples had a common origin, but it does not combine easily with other evidence because it does not tell the factfinder how likely the evidence would arise given an alternative hypothesis. Evidence of nonmatching is likely to be so powerful, however, that this concern will not be very substantial.<sup>43</sup>

Far more commonly, the critical evidence presented by a prosecutor is that DNA samples do match, and that the frequency of the profile revealed by the crime scene sample is very rare in a given population. In such a case, the factfinder is given sufficient information to form a workable likelihood ratio. The probability of the two samples satisfying the match criteria, given the hypothesis that they have a common origin, is close to 1, and the probability that they would satisfy the criteria given that they do not have a common origin and that the crime scene sample was left by some member of the given population, is  $1/F$ , where  $F$  is the frequency of the profile in that population. A factfinder disposed to treat the evidence of a match in a Bayesian way might therefore use a likeli-

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42. See Aitken, p. 237 ("[T]he likelihood ratio . . . is easily incorporated with other evidence.").

43. For example, in an important DNA case discussed both by Aitken (pp. 230-34) and by Robertson and Vignaux (pp. 168-71), *People v. Castro*, 545 N.Y.S.2d 985 (Sup. Ct. 1989), the prosecution had to disprove the hypothesis that blood found on the wristwatch of the defendant was the defendant's own. The prosecution was able to do this without significant difficulty. See Robertson & Vignaux, p. 169.

hood ratio of approximately  $F$ . This approach does not use the data as fully as does an assessment of the likelihood ratio without the interposition of the match concept, but it does allow for sensible combination of the DNA evidence with other evidence in the case.<sup>44</sup>

### III. LIMITATIONS ON THE LIKELIHOOD-RATIO APPROACH

The likelihood ratio is often crucial in assessing the probative value of evidence; however, it is also subject to limitations, three of which I discuss below.<sup>45</sup>

#### A. *The Direction of Causality*

Both Aitken and Robertson and Vignaux begin by discussing Locard's principle, that every contact leaves a trace (Aitken, p. 1; Robertson & Vignaux, p. 3). Most of their subsequent discussion concerns what is termed *trace* or *transfer* evidence.<sup>46</sup> A great deal of the evidence that is presented in court fits this form because it was created by the events in dispute. Thus, the broken glass at the scene of the burglary presumably was created by the burglary, and the glass in Dwyer's shoes was deposited there as a result of either his presence at the burglary or some alternative event that might be hypothesized. Even testimony might be considered trace evidence; some event (whether accurately described in the testimony or not) has occurred and led to the creation of the testimony.

In each of these cases, the chronology, and causation, runs from the hypothesis being tested or its negation to the creation of the evidence. Thus, we are able to say sensibly: "If Dwyer were at the scene, it is likely that the smashing of the window would leave in his

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44. The matching approach divides the possible measurements artificially into aggregates called "bins" and determines the probability, under each of the competing hypotheses, that the two samples would have measurements falling into the same bin. By contrast, what I have termed the likelihood ratio approach treats as different each measurement that is possible, given the phenomenon being measured and whatever level of sensitivity the measuring device has. It therefore might be called the *continuous* likelihood ratio approach; in a sense, it uses the smallest possible bins that measuring sensitivity allows, which means that even samples with a common origin are likely to yield measurements falling into different bins.

45. Robertson and Vignaux also discuss another problem: sometimes there is no "probability model" to determine the probability of the evidence given an alternative hypothesis. For example, forensic scientists have no basis for quantifying the probability that a given fingerprint would be created assuming the suspect did not leave it. Based on their experience, an expert might determine categorically that the print matches that of the suspect, in which case she implicitly treats as infinitesimally small the probability that anyone else left it. Robertson and Vignaux regard this situation as "unsatisfactory" and say that "further work is required in order to understand the processes involved in making these decisions." Robertson & Vignaux, p. 59. "In the meantime," they concede, "the proposal that all forms of scientific evidence be given in the form of a likelihood ratio is a counsel of perfection." Robertson & Vignaux, p. 59.

46. See Aitken, p. 1 ("For the most part the evidence to be evaluated will be so-called *transfer* evidence.").

shoes glass fragments of the type that actually were found there, and if he were not at the scene it is far less likely that events would lead to the deposit of such glass in his shoes. The likelihood ratio of the evidence is therefore high, and so the glass makes it substantially more likely that Dwyer was at the scene.” Not all evidence fits this mold, however. Sometimes chronology and causation run from the evidence to the hypothesis, rather than the other way around. In a murder case, for example, the prosecution may offer evidence tending to prove that at some point before the killing something happened that gave the defendant a strong incentive to kill the eventual victim. Motive might have caused murder; murder did not cause motive. It does little good in such a case to speak of the likelihood ratio — the probability of the motive-creating event given the proposition that the defendant committed the murder divided by the probability of that event given the proposition that the defendant did not commit the murder.

This is not to say that an observer could not assess the likelihood ratio: given a hypothesis, its negation, and a piece of evidence, an observer can always make an assessment, however tentative, of a likelihood ratio. But to do so in this case would be a vain exercise, requiring the conditional to be transposed twice, back and forth, unnecessarily. We begin with a causation-based sense of the probability of murder given motive. To determine the likelihood ratio, though, we would have to calculate the probability of motive given murder and the probability of motive given absence of murder. And for what purpose? So that we could transpose the conditional back to where we started and calculate the probability of murder given motive.

Probably most — though not all — quantifiable evidence is trace evidence, when causation, and so too our most intuitively accessible probability assessments, runs from hypothesis to evidence.<sup>47</sup> The fact that the relationship sometimes runs the other way cautions us that the likelihood ratio is not universally useful — and that it may not be a good abstract measure of probative value.<sup>48</sup>

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47. Suppose, for example, that paternity of a fetus is at issue and that two men are the only contenders. The frequency with which each man had intercourse with the woman around the presumed time of conception is a relevant piece of evidence. The causative relationship is that, all other things being equal, the more frequently a man has intercourse with a woman at the critical time, the more likely pregnancy is to result — not that if a man and a woman conceive a child it becomes more probable that they had frequent intercourse at the critical time.

48. In a given situation, the higher the likelihood ratio of the evidence with respect to a proposition, the more probative the evidence is with respect to that proposition. This does not, however, necessarily suggest that the probative value of the evidence should be *defined* in terms of the likelihood ratio. That would mean that we would treat the ratio, which we may express as the ratio of posterior to prior odds, as constituting the measure to be used in comparing probative value of evidence across situations, and not only in comparing the probative value of different pieces of evidence in the same situation.

## B. Multiple Alternative Hypotheses

Until now, I have discussed matters mainly as if there were only two available hypotheses — an affirmatively articulated hypothesis and its negation. Much of the discussion of all three books proceeds on the same basis,<sup>49</sup> though of course the authors know that this is a great simplification.<sup>50</sup> The difficulty is that the negation, and for that matter the affirmative hypothesis, consists of many, perhaps infinitely many, subhypotheses, and these do not all necessarily relate the same way to the evidence in question.

Recall the hypothetical car heading off the highway to the road that leads both to the airport and to a swimming place. If we wish, we can term  $H$  the hypothesis that the car is headed to the airport and  $Not-H$  the alternative hypothesis that the car is headed elsewhere. Obviously, though, a finer-grained analysis is possible. Call  $H_1$  the hypothesis that the car is headed to the airport;  $H_2$  the hypothesis that the car is headed to the swimming place along the same road;  $H_3$  the hypothesis that the car is headed to some other place the most direct path to which requires taking this exit; and  $H_4$  the hypothesis that the car is headed to some place the most direct path to which does *not* require taking this exit. Now, the evidence — the fact that the car has taken this exit — appears to make  $H_1$  more likely as compared to  $H_4$ , because the evidence is more likely to arise given  $H_1$  than given  $H_4$ . The evidence does not, however, appear to make  $H_1$  substantially more probable as against  $H_2$  or  $H_3$  because the evidence appears about as likely to arise given  $H_1$  as given  $H_2$  or  $H_3$ . So, how much does the evidence alter the relative probability of  $H_1$  and  $Not-H_1$ , which is the same as [ $H_2$  or  $H_3$  or  $H_4$ ]?

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Schum approves of the use of the likelihood ratio as a measure of probative value in part because it is insensitive to one aspect of the situation — the prior belief in the proposition at issue. See Schum, p. 217 (“[A]n item of evidence whose force is graded in terms of the ratio of posterior to prior odds will have the same measured force in changing a prior belief regardless of the strength of a prior belief.” (emphasis omitted)). In my view, whatever the merits of this insensitivity in other fields, it is a disadvantage in measuring probative value in litigation. If, taking the other evidence into account, a proffered piece of evidence alters the odds of a proposition from 1:1,000,000 to 1:250,000, I think we probably should deem it to be less probative than if it altered the odds from 1:2 to 2:1. I have debated the question of how to measure probative value — a question that is perhaps sterile and certainly arid — at some length with David Kaye. See Richard D. Friedman, *A Close Look at Probative Value*, 66 B.U. L. REV. 733 (1986); Richard D. Friedman, *Postscript: On Quantifying Probative Value*, 66 B.U. L. REV. 767 (1986); D.H. Kaye, *Quantifying Probative Value*, 66 B.U. L. REV. 761 (1986).

49. See, e.g., Robertson & Vignaux, p. 31 (chapter title: “The Alternative Hypothesis”); p. 42 (“[A]lthough the value of the evidence is decreased if the alternative perpetrator is a brother, so is the pool of possible suspects.” (emphasis added)); p. 48 (“[S]hould the alternative hypothesis assume that the child comes from a stressful and dysfunctional family?” (emphasis added)).

50. See, e.g., Aitken, pp. 110-15 (recognizing the simplification and illustrating a method for weighing evidence for more than two hypotheses).

The answer depends in large part on the prior probabilities of  $H_2$ ,  $H_3$ , and  $H_4$ . If before receipt of the evidence,  $H_4$  appeared to be by far the most likely of these — the bulk of traffic heads to places not calling for the airport exit, and we have no basis on which to regard this car as atypical — then the evidence will have substantial significance, because it will tend to make  $H_4$  much less probable. If, on the other hand,  $H_2$  appeared to be far more probable, because the occupants of the car were wearing bathing suits and there is no swimming place nearby except the one by the airport road, then the evidence will not have much probative value; it is what would be expected whether the occupants were going swimming or, for reasons presumably best known to themselves, heading to the airport in their bathing suits.

Now, instead of this somewhat contrived hypothetical, consider DNA evidence again.  $H_1$  is the proposition that the defendant is the source of a blood stain found at the crime. Assuming for simplicity that the stain is indisputably human blood, *Not- $H_1$*  is the proposition that the source was some person, other than the suspect, who was alive at or before the time the stain was created. Now, obviously *Not- $H_1$*  may be decomposed enormously: it comprises a subhypothesis for every human being alive at the time the stain was created and then some (assuming that relatively recent corpses might produce blood for a stain). The prior probabilities for each person within that large set are plainly not all the same; people who live near the crime or who knew the victim or who had some apparent motive to commit the crime are generally more likely than others to have done it. Moreover, the likelihoods — the probabilities of the evidence given each of the various hypotheses — are not the same for each person. Think of a series of concentric rings. The DNA of identical twins is identical, so that a sample left by the suspect's identical twin is as likely as one left by the suspect himself to reveal a given DNA profile; the DNA profiles of family members tend to resemble each other more than do those of strangers — a tendency that is generally stronger the closer the family relationship; and DNA profiles tend to resemble each other more for members of the same ethnic group than for members of different groups.

Because of this genetic consideration, it is analytically incomplete and often unsatisfactory to define a single alternative hypothesis, that someone other than the suspect left the sample found at the crime scene, and calculate a likelihood ratio on that basis. This totally aggregated approach creates significant distortion because it depends implicitly on the assumption that the identity of that other person does not affect the likelihood that his or her blood would reveal the profile that the crime scene sample does.

The most intellectually rigorous approach, a totally disaggregated one, would require computing not one likelihood ratio but rather several billion, one for each member of the human population. Of course, some of the likelihood ratios would matter very little, because the prior probability is minuscule that a person who has never left a small corner of the other side of the world would have committed this murder so far away from home. The result would be evidence of this form: The likelihood that a given blood sample would reveal the same DNA profile as that of the sample found at the crime scene is  $L_1$  times greater on the assumption that the sample was the suspect's blood than on the assumption that it was the blood of Person 1,  $L_2$  times greater if it were the suspect's blood than if it were the blood of Person 2 . . . and  $L_{5,000,000,000}$  times greater if it were the suspect's blood than if this were the blood of Person 5,000,000,000.<sup>51</sup>

Obviously, the evidence cannot be prepared, presented, or digested in this form. We must simplify greatly. The most appropriate method may be to aggregate to an intermediate degree. Under this approach, an analyst batches alternative hypotheses together, when this can be done without undue distortion, and determines a separate likelihood ratio for each significant alternative hypothesis or batch of alternatives. Suppose that for a large number of hypotheses  $H_i$ ,  $P(E|H_i)$  is virtually identical. Then these hypotheses may be batched together. Thus, we might say: "The likelihood that a blood sample would reveal the DNA profile revealed by the sample found at the crime scene is  $L_c$  times greater on the assumption that it was the suspect's blood than on the assumption that it was the blood of an unrelated Caucasian person,  $L_a$  times greater if it were the suspect's blood than if it were the blood of an unrelated African-American person,  $L_b$  times greater if it were the suspect's blood than if it were the blood of an unrelated Hispanic person, and  $L_d$  times greater if it were the suspect's blood than if it were the blood of the suspect's brother."<sup>52</sup>

A few points warrant emphasis. First, we can disregard alternative hypotheses that, even when aggregated, are highly improbable

51. See David J. Balding & Peter Donnelly, *Inferring Identity from DNA Profile Evidence*, 92 PROC. NATL. ACAD. SCI. USA 11,741, 11,742-43 (1995) (presenting a theoretical model in which "there is a distinct term for each possible culprit," so that "[o]ne need not consider any hypothetical 'random' individual, a concept that has led to considerable confusion").

52. Robertson and Vignaux write:

Theoretically, there can be an infinite number of different explanations for an event; it would be impossible to compare the prosecution's hypothesis with all of them. In practice, we can usually identify a small number worth considering. . . . [A]lthough it is the task of the prosecution to prove its case (that is, its assertion or hypothesis) beyond reasonable doubt we can judge whether it has done so by comparing its case with a small number of alternatives and, frequently, with just the one offered by the defence . . . .

Robertson & Vignaux, p. 34.



because the prior probability is low and the likelihood ratio is presumably high. For a murder in Boston, there is generally no need to inquire into the DNA characteristics of Navahos, unless there is some plausible reason to suspect that a Navaho might have committed the crime.<sup>53</sup>

Second, as both Aitken and Robertson and Vignaux stress, the *nature* of the alternative hypotheses to be examined generally does not depend on what we know about the *suspect*.<sup>54</sup> These are, after all, alternative hypotheses, under each of which someone *other than* the suspect is the source of the blood found at the crime scene. Of course, we may know something about the *criminal* to a high degree of probability, such as his race; if so, the hypotheses that would deserve serious consideration all would involve persons sharing that characteristic. Also, the *likelihood* of alternative hypotheses may depend on what we know about the suspect. David Balding and Peter Donnelly have stressed that because of "positive correlations in profile possession induced by shared ancestry," the probability is greater that another member of the suspect's ethnic group will have a DNA profile resembling that of the suspect precisely because the suspect's blood has that profile.<sup>55</sup> The importance of this effect is an actively disputed matter.<sup>56</sup>

Third, if likelihood ratios are used to compare two hypotheses that are not collectively exhaustive, we should not speak about the impact of the evidence on the odds of a given hypothesis, but rather about its impact on the relative probabilities of the two hypotheses. The principle is the same, however:<sup>57</sup> the prior ratio of the probabilities of the two hypotheses, assessed without the evidence, multiplied by the likelihood ratio, equals the posterior ratio of the probabilities of the hypotheses, assessed after the evidence. Thus, if we define  $L_{ij}E$  to be the likelihood ratio of evidence  $E$  with respect to hypotheses  $H_i$  and  $H_j$ , it is easy to show that

$$\frac{P(H_i|E)}{P(H_j|E)} = \frac{P(H_i)}{P(H_j)} \cdot L_{ij}E. \quad (7)$$

Indeed, in principle there is no reason why likelihood ratios of more than two hypotheses may not be expressed. For example, one

53. See Aitken, p. 215 (citing B. Devlin et al., *Statistical Evaluation of DNA Fingerprinting: A Critique of the NRC's Report*, 259 SCIENCE 748 (1993)).

54. See Aitken, p. 215; Robertson & Vignaux, p. 44 ("[T]he view has been put forward that the value of the scene DNA evidence may be affected by the characteristics of the accused's race. This is never true.").

55. Balding & Donnelly, *supra* note 51, at 11743.

56. See David J. Balding & Peter Donnelly, *Inference in Forensic Identification*, 158 J. ROYAL STAT. SOC., SER. A 21, 49 (1995) (responsive comments of B.S. Weir).

57. Indeed, if the two hypotheses are collectively exhaustive, these two forms amount to the same thing, because the odds of a hypothesis equal the probability of the proposition divided by the probability of its negation.

might say that a likelihood ratio  $L_{ijk}E$  of  $1:(1/2):(1/3)$  means that  $E$  makes the ratio of the probability of  $H_i$  to that of  $H_j$  two times greater, and the ratio of the probability of  $H_i$  to that of  $H_k$  three times greater, than they were without the evidence.

### C. *The Effect of Cascading*

Schum emphasizes the notion of cascaded inference: it may be that what we know for sure is not  $E$  but  $E^*$ , which tends to make  $E$ , and so in turn  $H$ , more likely. For example, we do not know for sure what DNA profile a given blood sample has, or even what the test revealed, but only what the laboratory has reported, and the lab may be in error. Schum shows that in such a case we need to know more about the relationship between  $H$  and  $E$  than just the likelihood ratio,  $P(E|H)/P(E|Not-H)$ . We also need to know the absolute value of the components,  $P(E|H)$  and  $P(E|Not-H)$ . The smaller they are, all other things being equal, the less valuable evidence  $E^*$  is (Schum, pp. 306-08). If the components are small, then  $E$  tends to be a rare event, making *Not-E* more probable;  $E^*$  therefore tends to be noisy evidence of  $E$ , and therefore also of  $H$ .

I suspect, though, that the problem is not always very serious. If we define  $E$  as a very rare event, such as two samples sharing a precise profile, then there is probably a range of *near-Es* — events that closely resemble  $E$  but are not identical to it — for which  $P(\text{near-}E|H)$  is substantially greater than  $P(\text{near-}E|Not-H)$ , and for which  $P(E^*|\text{near-}E)$  is substantially greater than  $P(E^*|\text{nowhere-near-}E)$ . In such a case, the value of  $E^*$  may depend only very slightly on the absolute values of  $P(E|H)$  and  $P(E|Not-H)$ . Perhaps the concept of fuzzy probabilities is helpful to analyze this situation, but I am not sure it is necessary.

In some cases, however, the error rate poses a substantial problem. Suppose a laboratory determines that a crime scene DNA sample and a suspect sample resemble each other closely, and the prosecution offers the hypothesis that they have a common origin. The defense might, of course, offer the alternative hypothesis that the person who left the crime scene sample happened to have DNA sufficiently similar to the defendant's that the two samples yielded similar profiles. Some research suggests, however, that by far the most important alternative hypothesis is that the laboratory committed error.<sup>58</sup> If this is so, I do not think that we can gain much comfort from Robertson and Vignaux's assertion that "[i]f evidence

58. See Jonathan J. Koehler, *Error and Exaggeration in the Presentation of DNA Evidence at Trial*, 34 JURIMETRICS J. 21, 24-26 (1993) (describing proficiency tests and their results); Richard Lempert, *Comment: Theory and Practice in DNA Fingerprinting*, 9 STAT. SCI. 255, 257 (1994) ("[T]he incriminating value of a DNA match can never be greater than the false positive error probability. . . . [T]he random match probabilities DNA evidence yields are smaller than any plausible false positive rates by many orders of magnitude."); cf.

is reported in likelihood-ratio terms then all the possibilities for error should be incorporated into the likelihood ratio" (Robertson & Vignaux, p. 96). The likelihood ratio must take into account the possibility that the evidence actually presented — the laboratory report — results not from the two samples having a common source, or from a coincidental resemblance in DNA, but from mere error. And that possibility may be a hard one to quantify precisely.

#### D. Demands for Data

The likelihood-ratio approach focuses on the precise evidence that has been presented and asks how likely that evidence would have arisen under each of the competing hypotheses. One question, for example, may be how likely it would be, if the crime scene sample were left not by the defendant but by a member of some given population, that probe D1S7 would indicate fragment lengths of 9024 and 2166 base pairs.<sup>59</sup> To provide a satisfactory estimate of that probability requires a great deal of information. The task is analogous to determining the frequency of people within a given population who are between 1.79995 and 1.80005 meters tall. We can have some confidence, however, that the distribution of heights is rather smooth, so that data about a larger interval provide a good basis for estimation. DNA distributions may be spikier; even if it is appropriate to smooth out the data to some extent, the assumptions used in accomplishing this task, which Aitken demonstrates,<sup>60</sup> may affect the results quite dramatically.<sup>61</sup>

#### IV. PRESENTATION

Suppose now that the problems discussed in Part III are not glaring: the causal relationship runs from hypothesis to evidence, the alternative hypotheses may be batched together into a tolerably

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Kathryn Roeder, *DNA Fingerprinting: A Review of the Controversy*, 9 STAT. SCI. 222, 244 (1994) (presenting a more favorable view of the laboratory-error statistics).

59. I have taken this example from Aitken, p. 216.

60. See Aitken, pp. 233-34; cf. Balding, *supra* note 9 (criticizing Aitken's advocacy of kernel-density estimation because "the underlying density is, due to population genetics, very spiky"); Roeder, *supra* note 58, at 241-43 (favoring the practicality of smoothing functions in using the likelihood ratio approach).

61. In part because of the "detailed information" that a likelihood ratio approach would require, a committee appointed by the National Research Council (NRC) rejected this approach without full discussion. See NATIONAL RESEARCH COUNCIL, DNA TECHNOLOGY IN FORENSIC SCIENCE 62, 85 (1992). A new committee appointed by the NRC to reexamine certain issues related to DNA evidence, including this one, issued its report shortly before publication of this essay. See NATIONAL RESEARCH COUNCIL, THE EVOLUTION OF FORENSIC DNA EVIDENCE (1996). This report explores advantages and disadvantages of such an approach and, without recommending its adoption, makes some suggestions as to how it might be implemented. Cf. Lempert, *supra* note 58, at 258 (arguing, as a former member of the 1992 committee, that "Bayesian approaches have much to offer in this area" and that the committee had "no adequate justification" for rejecting them).

small set without much distortion, lab error is not a significant problem, and a witness can provide satisfactory assessments of the likelihood ratio or of its components. Then it seems that, ideally, the evidence should be presented in likelihood ratio terms.

Perhaps the expert witness simply should present the likelihood ratio itself, as Robertson and Vignaux suggest (Robertson & Vignaux, p. 21). Assuming that only two hypotheses warrant attention, the testimony might take the form: "This evidence is 1700 times more probable if the accused left the mark than if someone else did."<sup>62</sup> Or perhaps the witness should present the actual components of the ratio,  $P(E|H_1)$  and  $P(E|H_2)$  for a two-hypothesis case: "If the accused left the mark, the probability of the evidence meeting this description would be .17, and if anyone else left the mark the probability of the evidence meeting the description would be .0001."

Certainly, in a case like this, the expert should not testify to  $P(H|E)$ , because that involves an assessment not only of the likelihood ratio but of the prior probability,  $P(H)$ , which depends on all the other evidence in the case. This is a principal theme of Robertson and Vignaux, who defend the traditional rule barring witnesses from testifying to the "ultimate issue" (Robertson & Vignaux, pp. 60-65). Aitken, less expressly, supports it as well.<sup>63</sup>

There are two basic problems, however. First, not all expert witnesses are reliably capable of the verbal precision necessary to present the likelihoods, or their ratio, correctly.<sup>64</sup> Second, even when they are, jurors — and many people more sophisticated than the average juror — often will misunderstand.<sup>65</sup> Probably the most sig-

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62. If more alternative hypotheses seem significant, the witness could make further statements in this form, one for each alternative.

When there are multiple alternative hypotheses, the expert also might offer one likelihood ratio but then disaggregate the denominator. In other words, after stating the overall likelihood ratio, the expert might say something like, "If the crime scene sample was left by a Caucasian person other than the accused, it would be twice as likely to yield this DNA profile as it would be if it were left by an African-American person, and three times as likely as it would be if it were left by a Native American person." The trouble is that the expert could not compute the denominator of an overall likelihood ratio on the basis of this information; she also must assess the prior probabilities — that is, the probabilities assessed without regard to the DNA evidence — of each of the alternative hypotheses. That, as discussed below, is often an inappropriate role for the expert to play. See *infra* text accompanying note 63.

63. See Aitken, p. 225. In some cases, though, when the expert is asked on the witness stand to make the type of judgment she ordinarily makes outside the litigation context, I believe abrogation of the ultimate-issue rule probably makes sense. See generally FED. R. EVID. 704 (abrogating the rule, with one later-added exception).

64. See Robertson & Vignaux, p. 92; Balding & Donnelly, *supra* note 56, at 42 ("Explaining the distinction between the correct and incorrect probability statements at court is excruciatingly difficult (and I speak from experience)."); Koehler, *supra* note 58, at 28-31.

65. See Bruce C. Smith et al., *Jurors' Use of Probabilistic Evidence*, 20 LAW & HUM. BEHAV. 49 (1996) (reporting on experimental studies in which mock jurors tended to underuse evidence, as compared to the Bayesian norm, and in which, in contrast to prior experiments involving more powerful probabilistic evidence, relatively few succumbed to the

nificant difficulty, in both expert articulation and juror comprehension, is the fallacy of the transposed conditional — confusion of  $P(E|H)$  with  $P(H|E)$  and  $P(E|Not-H)$  with  $P(Not-H|E)$ . Applying the fallacy to both the numerator and denominator of the likelihood ratio transforms the ratio into  $P(H|E)/P(Not-H|E)$ , the odds of  $H$  given  $E$ .

I suspect the problem sometimes is a *failure* to transpose the conditional. For example, if a juror is presented with evidence in the form, “ $P(E|H)$  is high and  $P(E|Not-H)$  is low,” an intellectual laziness might prevent the juror from going through the Bayesian transformation necessary to determine  $P(H|E)$ . Part of the problem may be insufficient verbal acuity. It is hard to express the proposition “ $P(E|H)$  is much higher than  $P(E|Not-H)$ ” in any compact verbal form that does not make it sound much like “ $P(H|E)$  is much higher than  $P(Not-H|E)$ ,” and it is hard for a listener, or even a reader, to make the distinction.

Another part of the problem may be that  $P(E|H)$  and  $P(H|E)$  bear a close relationship to one another. Equation 2 shows that  $P(E|H) = P(E \text{ and } H)/P(H)$ , and  $P(H|E) = P(E \text{ and } H)/P(E)$ . The numerator in each fraction is the prior probability of the conjoint occurrence of  $E$  and  $H$ . In terms of the diagrams presented earlier,  $P(E|H)$  equals the  $[E \text{ and } H]$  area divided by the  $H$  area, while  $P(H|E)$  equals the same  $[E \text{ and } H]$  area divided by the  $E$  area. If  $P(E)$  equals  $P(H)$ , then  $P(E|H)$  equals  $P(H|E)$ . This, it easily can be shown, happens in one particularly seductive case, when the prior odds of  $H$  are even and  $E$  appears to be as good an indicator of  $H$  as  $Not-E$  is of  $Not-H$ .<sup>66</sup>

Whatever the reason for the difficulty, we are going to have to deal with it for some time. One would like to think that forensic scientists can learn to deal with elementary probability theory; lawyers might take longer, and jurors — the general public — longer still. Perhaps a part of the solution is the proposal advanced by Ian Evett, and presented sympathetically by both Robertson and Vignaux and Aitken, of using a standardized set of words to express likelihood ratios of various strengths — for example, *very strong* for a ratio of more than 1000, *strong* for a ratio of 330 to 1000, and so forth (Aitken, p. 52; Robertson & Vignaux, pp. 56-57). But, as Robertson and Vignaux argue, without the numbers we lose a great

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“prosecutor’s fallacy,” a form of the fallacy of the transposed conditional); Amos Tversky & Daniel Kahneman, *Extensional Versus Intuitive Reasoning: The Conjunction Fallacy in Probability Judgment*, 90 PSYCHOL. REV. 293, 300 (1983) (finding that the transposed-conditional fallacy was common even among graduate students of social sciences, though not as common as among undergraduates).

66. That is,  $P(H) = .5$  and  $P(E|H) = P(Not-E|Not-H)$ , which by definition equals  $1 - P(E|Not-H)$ .  $P(E) = [P(H) \cdot P(E|H)] + [1 - P(H)] \cdot P(E|Not-H)$ , so in this case it equals  $.5P(E|H) + .5[1 - P(E|H)] = .5$ .

deal of information (Robertson & Vignaux, p. 57). Nor is it clear that we would avoid much confusion by using the numbers alongside the standard verbalizations. The presentation problem is tenacious.

### CONCLUSION

One of the driving themes of the Robertson and Vignaux book is that when scientific evidence is presented in court, the most difficult problems are likely not to be questions about the underlying science, or even about proficiency of testing, but about “the interpretation of the evidence” (Robertson & Vignaux, p. 109), “inference and reasoning” (Robertson & Vignaux, p. 173). I am persuaded by these books that Robertson and Vignaux are probably (there I go again) correct — or to put it in fuzzy terms, correct to a large degree. They contend further that “logic, probability and inference provide the language in which [lawyers and scientists] should communicate with each other” (Robertson & Vignaux, p. 217). I agree, though, that this is more easily said than done. Each of these books is a significant contribution to the doing, to opening that channel of communication.